THE TELEGRAPHIC JOURNAL.

VOL. II.—No. 37.

THE ELECTRIC CONDUCTIBILITY OF LIGNEOUS BODIES.*

By the COUNT DU MONCEL.

(Continued from p. 262.)

THIS second part of my researches, having in the previous part shown the general effects of the conductivity of ligneous bodies, comprises the examination of the manner in which the electric propagation behaves in these kinds of imperfect conductors.

By reason of the considerable resistance presented by ligneous substances, the influence of their form and of the arrangement of the plates of communication ought to be revealed in a notable manner in the obtained results, so that I could not expect to trace in the electric transmissions across the woods—whose length and section I varied—the simple laws of Ohm. This indeed is what experiments have proved; but, in order precisely to specify these effects, it is first necessary to calculate the resistance of the ligneous bodies to the propagation of the current, which, with so sensitive a galvanometer as mine, and for want of any rheometer or measuring apparatus of great resistances, was not an easy affair to ascertain. I was obliged to have recourse to an indirect method, by laying the "System of Derivations" under contribution, and by drawing up a table to indicate the resistances which, under the conditions of my experiments, would furnish such and such deflections on my galvanometer. For that purpose I joined the two extremities of my galvanometer wire by a wire of 100 metres of resistance (Siemens's unit), and having placed in the battery circuit, with a total resistance of 2048 kilometres of telegraph wire (2048 units), I joined the two disunited extremities of this circuit to the two ends of the galvanometer wire. By this means the current of the battery (used in all my experiments) after having crossed traversed a resistance capable of being varied, it was easy for me—by causing the galvanometer deflection to equal the degree marked by the current directly traversing the wood—to obtain to equations from which could be deduced the value of the unknown wood-resistance.

Let R represent the rheostat's resistance; d the branch resistance; g the galvanometer's resistance, equal to 733 Siemens's units; I, current's strength; E, electromotive force; r, battery resistance. Then—

$$I = \frac{E\,d}{(r+R)(g+d)+g\,d} = \frac{E}{r+g+x},$$

whence—

$$x = \frac{g(r+R)}{d} + R.$$

Now experiment proved that, under the conditions of my work, for a deflection of—

	R = Kilos.	x = Kilos.
12°	2048	15,057,868
14	1536	11,304,396
16	1280	9,427,660
18	1024	7,550,924
22	768	5,674,188
26	640	4,735,820
30	512	3,797,452
36	384	2,859,084
41	320	2,389,900
46	256	1,920,716
53	192	1,451,532
57·5	160	1,216,940
62·5	128	982,348
67·5	96	747,756
74	64	513,164
77·5	48	395,868
81·5	32	278,572
84	24	219,924
85·5	16	161,276

Once in possession of these data I was able, either by interpolation or by every time repeating the experiment for each new case that presented itself, to deduce the resistance of all my lengths of wood.

On experimenting several times at different stages of moisture with four rulers of oak, 1·20 m. in length, 3 centimetres in width, and 6 m.m thick, I found, for mean deflections corresponding to lengths doubling from 15 c.m. to 1·20 m., the following figures:—

Length.	°	
1·20 m.	63·1	11·9
0·60	75·4	15·6
0·30	81·2	22·8
0·15	85·0	32·5

The resistances of the wood corresponding to the first of these deflections being 982348, 513164, 278572, and 161276, we obtain for ratios of the corresponding electric current strengths, after adding $g+r=739$ kilos.,—

1·91 1·85 1·72

which should have been 2 according to Ohm's simple law. For the second deflections the ratios are—1·60; 1·72; 1·61.

Thus it is seen that the law which concludes that the strengths are inversely as the lengths is rather disguised, and that so much the more as the wood is dry.

The same is the case when we seek to verify the law with respect to sections. Thus, on successively placing upon each other two and four of the wooden rulers used in the preceding experiments, taking care to so moisten as to furnish separately nearly the same deflections, the following deflections were found:—

11·9° 14° 21·3°.

Now the ratios of the strengths corresponding to these deflections are 1·33, 2·65. Theoretically they should have been 2 and 4.

There is, then, a cause which tends to weaken the value of the ratios that ought to be found, and this cause, independently of the inequality of action occasioned by the dampness of the different parts of the wood (not being the same in the interior as on the surface), is the *breadth* and the *thickness* of the wood with regard to the connecting plates. These plates, for most of the experiments of which I have spoken, are very small (3 square c.m. each

* Communicated by the Author.

in surface), and do not cover the whole width of the wood; they are, moreover, applied parallelly to the electric transmission. Under these conditions the resistance in the direction of the width and thickness is not negligeable as with good conductors, and it ensues therefrom that *the interpolar resistance is much greater in the middle than in the neighbourhood of the surfaces*, and this augmentation should be more perceived the shorter the wood's length and the greater its thickness. On the other hand, we ought to trace from such an arrangement the effects of transmission in plates studied by MM. Kirchoff and Smaasen,—effects which, as is known, tend to efface the influence of distance between the connecting plates. Evidence of this action might be had by arranging the experiment so that the connecting plates should be of the same surface as the entire section of the superposed plates, and should be clasped at the extremities of these last perpendicular to their axis. On taking four prisms of oak wood, each 8 c.m. long, 3 c.m. wide, and 6 m.m. thick, and introducing them—in the direction of their length—between the two jaws of a wooden vice provided with ebonite plates to secure a good pressure of the electrodes, I was able—on taking away successively, by blows from a hammer, one, two, or three of these prisms—to vary the section without altering the conditions of the experiment. Now the means of the deflections supplied in these different instances were—for the unit section, 36·1°; for two prisms, 47·6°; for three prisms, 59·41°; and for all four, 65·06°. The corresponding resistances to these deflections, comprising $g+r=739$ kilos., being 2859823, 1814090, 1128140, 862783, the ratios of the three first compared with the last are found to be—

1·30 2·10 3·31;

which to be exactly conformable to the law of sections should be—

1·33 2 4.

There is, it is true, only a sensible difference in the last comparison; but I think it may be attributed to the increase of pressure which was concentrated on a single prism when the deflection 36·1° was taken, whereas when the deflection 65·06° was taken it was spread over the whole four. As the conductivity of the wood increased in large proportions with the pressure, it is probable that the extra pressure on the single prism rather exaggerated the corresponding deflection.

The influence of the size of the connecting plates, as well as their position with respect to the interpolar line and the direction of the wood's fibre, naturally busied me, and form the complement of my researches. I have indeed undertaken, in this respect, some interesting experiments, whose results I am about to add.

Thinking I could discover more or less in my experiments traces of MM. Kirchoff's and Smaasen's law, resulting from their formula respecting electric propulsion through space that the proportion of the electric intensities is as the square roots of the connecting-plates' surface, I cut out of a very thin sheet of platinum three series of circular plates, of 24, 47, and 92 m.m. radii, and 4, 16, 64 c.m. in surface. With similar plates, but of *double* the surface, I ought to find, if my supposition is correct, on comparing the two sets two and two, the ratios of resistance represented by 2. This is how the experiment was planned:—One end of an oak plank, 4 c.m. thick, 26·5 c.m. long, and 12·5 c.m. wide, was affixed by its central part to one of the electrodes, itself supported upon an ebonite plate by the medium of a small blotting-paper cushion. Above this plank, and in connection with the preceding electrode, was fixed the second electrode, and above that a second blotting-paper cushion and a second plate of ebonite. The whole was placed in a bookbinder's wooden press. The lever of the screw of the press was so disposed that the degree of pressure on the electrodes and the plank could be noted,—an indispensable condition, by reason of the enormous variations which unequal pressures involve in the wood's conductivity. When I had attained about the maximum squeeze I allowed my current to pass through the plank, and I noticed the deflections furnished by my galvanometer with the different systems of electrodes: pressure always the same, and at the same part of the plank. These experiments, I should say, are very delicate and very minute, and it was not until they were repeated several times that concordant figures were obtained. The means of the obtained deflections were the following:—

1, with 4 square c.m. electrodes	...	50°
2, „ 16	„	„	...	66°
3, „ 64	„	„	...	78°

By referring to the former table it will be found that the resistances corresponding to the deflections nearest to those given above are 1652611, 818297, 395868, to which, if $g+r$ be added, we obtain, as before explained, 2·02, 2·06, numbers in all respects agreeing with theory. One may then conclude that in the majority of cases the electric currents' strength across wood are proportional to the square roots of the surfaces of the connecting plates.

In the preceding experiments the electric intensities are pretty considerable, because the duration of the variable period of the electric propagation is imperceptible, or at least cannot be perceived in the midst of the needle's oscillations which precede its arrest; but with less feeble intensities it is not so. Thus, if we repeat the previous experiments with a weak pressure—for example, with such as would result from applying a weight of 20 kilos. upon the upper ebonite plate—we should have the following deflections with the electrodes of 64 c.m.:—

12°	... ...	after 1′
13°	... ...	„ 2′
14°	... ...	„ 3′
15°	... ...	„ 3′ 38″
14·5°	... ...	„ 4′
14°	... ...	„ 5′

With electrodes of 16 c.m. I obtained—

7·5°	... ...	after 1′
8°	... ...	„ 3′
9°	... ...	„ 4′
9·5°	... ...	„ 5′
9°	... ...	„ 6′

Is it solely to the delay occasioned by the variable period of the propagation to which must be attributed this progressive increase of the electric intensity, or to this delay combined with a better contact between the electrodes and the wood arising from a prolonged pressure? I incline to this latter hypothesis. This period of increase always augments with the resistance of the interposed con-

ductor, and, after a certain maximum, there is a decrease of intensity which can be scarcely interpreted according to Ohm's theory, and which can only be the result of polarisation.

To study the influence of the fibre's direction in the wood on the electric transmission, I had cut out, from one piece of reputed dry oak, two discs, 3 c.m. thick and 13 c.m. diameter, one of whose surfaces was parallel to the fibres, and the other perpendicular. Arranging the experiment with these two discs under precisely similar conditions of the previous tests, I obtained—with the electrodes 16 c.m. diameter—the following results:—

Disc with fibres perpendicular to the surface, 57·33°
" " parallel " " 68·7°,

which are the means of six successive and alternate experiments whose indications did not vary between them more than 1°. They show that the wood with parallel fibres conducted better than the other. But it is, however, necessary not to hasten to conclude thus, for I think this difference depends especially upon the difference of pressure exercised. The wood with perpendicular fibres is indeed scarcely susceptible of yielding under ordinary pressure, whilst the other is capable of yielding more easily. In the experiments in question the maximum pressure was developed, and the lever of the press presented a difference of position of nearly half a turn. When the pressure was close upon the same the "parallel" wood only gave a deflection of 60°. Nevertheless, desiring a more conclusive result, I tested with platinum electrodes 3 square c.m. of surface, and found the deflections were—with parallel fibres 39°, and with perpendicular fibres 24°. I repeated the same experiments with two oak prisms, of 10 c.m. length and 2 c.m. in the other sides, and so cut out that their length was parallel to the fibres of the wood for one prism; in the other the length was perpendicular. The same effects were obtained: thus the first of these prisms gave 13°, the other only 9°, and yet they had both been dried and moistened at the same time.

To complete my experiments I determined to assure myself if there were no method of isolating more perfectly the woods, by stopping their pores with an isolating and impermeable substance. In the first paper it will be seen that guaiacum, in consequence of treatment at a stove and of resin from it, had become relatively a good insulator; the same with the ivory, which, from the moment when it yielded the brown liquid, no longer gave any deflections, even after twenty-two hours sojourn in the moist receptacle. This liquid, which thickens as it cools, is quite insulating; and it is probable that in closing up the ivory's pores it hindered the introduction of the damp air to the interior of the pores, and rendered it completely insulating. Precisely the same was noticed with heated scales. But the major part of the woods does not come under these conditions, and, to make insulators of them, it is necessary to inject into them insulating matters—liquid and susceptible of solidifying. M. Niaudet Briquet has had the ingenious idea of injecting them with paraffin, and he sent me for trial several samples doubly injected,—some after a preliminary drying at the stove, and similar ones undried. I asked this because, with woods imperfectly dried, the moisture might be found imprisoned so as to prevent the paraffin from thoroughly penetrating their pores, and might consequently have ultimately allowed the moisture of the air to be absorbed.

When the samples were sent to me they gave no signs of conductivity, excepting the mahogany, *undried at the stove*, which gave a deflection of 12°. After twenty-two hours confinement in my damp receptacle I tested them afresh, with the following results:—

	Paraffined after Stove treatment.	Paraffined without Stove treatment.
Poplar	16°	18°
Acajou	10°	35°
Beech	8°	18°
Lime	8°	22°
Wych elm ...	8°	10°
Ash	6°	22°
Pear	6°	16°
White fir ...	6°	9°
Walnut	2°	9°
Oak	0°	5°

The above table proves that woods paraffined after treatment at the stove are infinitely smaller conductors than those not so treated; and it is probable that if they had been twice successively treated at the stove, and injected on coming from it whilst still hot, that no deflection would be produced.

THE MEASURE OF THE ELECTROMOTIVE FORCE OF BATTERIES IN ABSOLUTE UNITS.*

By M. A. CROVA.

In reducing into absolute units the electromotive forces of various elements obtained by the method of opposition, I have always found the results greater than those deduced from either Ohm's method or from the heat generated by the chemical action which takes place in the element. It is known, besides, that within certain limits the electromotive force depends on the strength of the current in the circuit, and that many physicists have attributed these variations to the polarisation of the negative plate.

In order to ascertain the figure we should adopt to represent the absolute electromotive force of an element, I used the following method:—

Call h, h', h'', ... the internal resistances, and i, i', i'' ... the correspondent values of the strength of the current produced by a determinate element. Instead of taking h and i as variables, trace the curve whose abscissæ are the values of i, and whose ordinates are the correspondent values of hi. We shall obtain a right line if the element is constant, and, representing by y the values hi, we shall have—

$$hi = y = A - ri,$$

the equation of a right line whose ordinate at the beginning exhibits the electromotive force, and whose angular coefficient is the resistance of the element.

If we obtain by experiments n values of h, corresponding to an equal number of values of i, we shall have n points of the line sought; and, if it is straight, we can draw from its construction the mean values of A and r, calculated by Ohm's formula, by means of $n-1$ couples of the n consecutive observations, grouped two and two. Indeed, on account of errors of observation, the n points of

* Paper read before the Academie des Sciences at Paris.

the obtained line will deviate here and there a very little from a *mean right line*, that may be unhesitatingly traced if the observations are well made. This method has, moreover, the advantage of demonstrating the limits between which the element may be considered a constant, and gives, outside these limits, the value of the variations.

In operating on Daniell's and Grove's elements I observed that this line is straight in almost its entire extent, but that it rises slightly in the neighbourhood of the axis of the y's, which it meets at an angle greater than r. I continued in these researches to make use of the units adopted in my first labours (recorded in the *Annales de Chimie et de Physique*, 3rd series, vol. lxviii., and 4th series, vol. iv.),—that is to say, the resistance at zero of a column of mercury 1 metre long and 1 square m.m. in section, and a current strong enough to decompose 9 milligrammes of water in one hour.

We will quote some numbers obtained in absolute units by multiplying them by a suitable factor:—

The electromotive force of a Daniell element, whose plates are each 40 square c.m. of surface, is, at a temperature of 11°, equal to 43·1.* It is constant whilst the resistance is sufficiently weak for the current to be superior to the unit. The strength varying from 1 to zero, the electromotive force increases from 43·1 to 43·9—a value obtained by M. Poggendorff's compensation method.

The electromotive force of a Grove's element, of similar dimensions and under similar conditions, should be constant and equal to 75·0, when the intensity of the current is superior to unity. The strength varying between unity and zero, the electromotive force increases from 75·0 to 78·0—value given by M. Poggendorff's method.

The resistance of Daniell's element varies from 5 to 15 metres, according to the concentration of the sulphate of zinc. That of Grove's element is about 1 metre. Now MM. Favre and Silbermann have found that 1 grm. of zinc, in uniting with copper in the proportion of $CuSO_4$, disengages 714 units of heat (*calories*). The absolute electromotive force, x, of Daniell's element will then be—

$$x = 714 \times 0{\cdot}000033858 \times 415{\cdot}41 \times 10^{10} = 11{\cdot}022 \times 10^{10}$$

in which 0·000033858 represents the electrochemical equivalent of the zinc in grammes, and $415{\cdot}41 \times 10^{10}$ the mechanical equivalent, in absolute units, of a *calorie* (gramme-degree). The electromotive force of a Daniell's element under circumstances which render it constant, being 43·1, we must, to convert it into absolute units, multiply it by 0·2666 (the value of my unit of intensity in Weber's Electro-magnetic Units), and by the absolute resistance of the mercurial unit. Now this last number is far from being exactly known, for the mean of the results (0·9629 and 0·9564) obtained by the British Association is $0{\cdot}9596 \times 10^{10}$; Weber gave 0·9749; and the recent labours of M. Lorenz and those of M. Kohlrausch have respectively given us 0·9337 and $0{\cdot}9717 \times 10^{10}$.

In the face of such divergent results I have calculated the value of the coefficient which it is necessary to adopt to render the calculated electromotive force equal to that deduced from the heat of chemical union. We have—

$$43{\cdot}1 \times 0{\cdot}2666\, x \times 10^{10} = 11{\cdot}022 \times 10^{10},$$

whence— $x = 0{\cdot}95891$,

a result which approximates closely to 0·9596, the mean of the British Association's results.

M. Bosscha obtained for the value of the e.m. of a Daniell's element, 10·258; M. Thomsen, 10·79. These differences appeared to me to point to a value of the absolute unit of resistance more or less inexact. The error of the number 11·38, obtained by M. Kohlrausch, seemed due to too great a value of the coefficient 0·9717.

A difference in the same sense is observed between the number 19·18, which represents the e.m. of Grove's element obtained by my graphic method and the coefficient 0·95891, and the number 19·42 given by M. Kohlrausch.

Lastly, I found by my method that the e.m. of a Smee's element, in which each plate has a surface of 9 square c.m., is comprised between a maximum of 34·529 and a minimum of 31·709. In converting these numbers into absolute units, by means of the factors 0·2666 and 0·95891, we obtain respectively 8·109 and $8{\cdot}830 \times 10^{10}$. Now MM. Favre and Silbermann have found that 1 grm. of zinc, in uniting with hydrogen in the proportion of SO_4H, disengages 567·9 *calories*, and from it we may deduce the number $7{\cdot}9855 \times 10^{10}$ for the value of Smee's element, instead of 8·109 and $8{\cdot}830 \times 10^{10}$.

* In mercurial column measurement.

STATIC INDUCTION PHENOMENA PRODUCED BY THE RUHMKORFF COIL.

By M. E. BICHAT.

Ruhmkorff's coil is, undoubtedly, the best apparatus to transform dynamic into static electricity: reciprocally this same coil should be the most suitable apparatus for the inverse transformation of static into dynamic electricity. Indeed, if a series of static-electric sparks, produced from a Holtz machine, are made to pass into the fine wire of the coil, currents are induced in the thick wire which excite attention from the extreme facility with which they decompose water and salts. Theoretically one should expect to induce two currents equal in quantity. Now there is nothing of this: it is known that, the experiment being made with the largest induction coils constructed by M. Ruhmkorff, on the one side of the voltameter almost pure hydrogen is always disengaged, and on the other side oxygen. If, instead of water, sulphate of copper is decomposed, at only one of the electrodes is the copper deposited, and the direction in which this decomposition is effected always indicates an inverse induced current. It seems then, according to this experiment, that there is but one current in contradiction with the known facts regarding induction; and it is this apparent contradiction which my researches have sought to dispel.

It was natural first to enquire what might be the influence upon the production of the phenomenon of the cluster of iron wires, which, in Ruhmkorff's coils, contribute in so powerful a manner to the transformation of dynamic into static electricity. I therefore procured a small coil whose soft iron core was not fixed. First the machine was worked without the core, then a bar and a bundle of soft iron wires were successively introduced into it, with the following results:—

When the coil is empty the current is always

weak, but perfectly appreciable, and it is inverse when an iron bar is introduced; the deflection of the galvanometer corresponding to the inverse current is much increased if the bar is replaced by the bundle of iron wires.

Besides, it is remarked that the gas disengaged at the voltameter's platinum electrodes is extremely weak and insignificant when the coil is empty, but it becomes quite appreciable on the introduction of the soft iron. In short, the gases disengaged, in the experiment made with the large coil, must be attributed almost totally to the influence of the soft iron.

If care is taken to examine attentively the conditions of the experiment, it will be easy to explain these phenomena so whimsical in appearance. Let us take, first, the experiments on the polarisation of the electrodes, and let us suppose the coil empty. It must be remarked, at the outset, that the current from Holtz's machine is obliged to traverse a very long circuit, of small diameter, of considerable resistance. The positive and negative electricities accumulate little by little on the balls of the excitor until the strength being sufficient the spark bursts forth.

We may more simply express this fact by saying that the spark commences slowly and finishes abruptly. There result two induced currents equal in quantity, but of very unequal potential. The potential of the direct current, which corresponds to the break of the primary current, is enormously opposite to that of the inverse current proceeding from the slow establishment of the spark which constitutes the inducing current. The inverse current decomposes the water in the voltameter slowly, and deposits on the platinum a very large quantity of microscopic gaseous bubbles, that are not disengaged, and consequently create polarising currents.

The direct current which afterwards arrives decomposes the water, but, as it lasts a very short time, the result is that this decomposition is effected very rapidly. The gas bubbles are larger, are disengaged immediately from the platinum, and, producing only a very feeble polarisation, are unable to destroy the polarisation of the inverse current.

The apparent production of a single inverse current is due, then, to the difference of potential of the two induced currents; and, hence, we ought to expect to see the galvanometer's deflection lessen if—by any means whatsoever—we can lessen the potential of the direct current: this may be done with the help of diaphragms. If we introduce into the empty coil a tube of continuous copper the galvanometer's deflection immediately decreases. It remains, on the contrary, the same as it was before if we replace the continuous tube with a slit one.

Let, now, a bar of soft iron be introduced into the coil. The phenomena produced will be the same as when the coil was empty; but at the same time the magnetisation of the soft iron will give rise to other induced currents in addition to the first. To comprehend thoroughly what takes place we must remember that of the two currents, direct and inverse, it is the direct which magnetises the strongest, because it has the highest potential. We may, then, say that the magnetisation produced by the first spark is direct. On the other hand, we know that a certain time is always necessary to demagnetise a piece of soft iron. It follows that the sparks of Holtz's machine, succeeding one another with rapidity, the magnetisation produced by the first will not be completely destroyed on the arrival of the next. This second spark adds its effect to that of the first, and augments the magnetisation of the soft iron.

The considerable augmentation of the effects produced by the inverse current when we replace the bar with a bundle of soft iron wires easily explains itself, if, in the phenomena of static induction, as well as in those of ordinary induction, the interposition of diaphragms lowers the potential. The demonstration of this fact I trust I have clearly shown.

To recapitulate:—If an intermitting battery current be made to pass into the thick wire of a Ruhmkorff coil, two currents of contrary directions are induced in the fine wire, and, for a certain explosible distance, there appears to be only one single current produced. This current is *direct*, and the sparks produced therefrom have in all respects the semblance to sparks from static electricity. Reciprocally, if into the fine wire of the coil a series of sparks from static electricity be made to pass, we induce, in the thick wire, currents in all points analogous to those furnished by the battery; and in studying these currents by means of a voltameter there seems to be only one current which is *inverse*.

ON THE NEW CONTACT THEORY OF THE GALVANIC CELL*

By J. A. FLEMING, B.Sc., F.C.S.

THE old contact theory of Volta had its origin in an entire ignorance of the science of energy. It simply referred the current produced through the circuit of a pile to the effect of the metallic contacts, and it ignored the thermal and chemical changes which are also necessarily present; but it had to be finally abandoned when once it became clearly understood that the appearance of a current involved the disappearance of some other energy, actual or potential, as an invariable accompaniment. The new contact theory may be said to have had its source in the discovery of Sir W. Thomson, that there is undoubtedly a difference of potential produced when dissimilar metals are placed in contact—a fact not only abundantly proved by Thomson by direct experimental evidence, but, as he has pointed out, confirmed in a remarkable way by the phenomena of the Peltier effect, which, when interpreted by the dynamical theory of heat, furnish the most reliable measure of its amount. These facts, together with others presently to be referred to, have been made to furnish the key to a fresh explanation of the dynamics of the galvanic cell, which I have ventured to call the *new* contact theory, as opposed to the old or voltaic one.

The object of the present paper is to draw the attention of those interested in this question to the objections that may be raised against this new contact theory,—objections based on facts, some old, and some which perhaps may prove new, but all of which alike seem to throw fresh difficulties in the way of this theory, although capable of simple explanation by the old chemical hypothesis. It will be necessary, then, to review briefly the precise statements of this new contact theory, in order to show exactly what are the points against which ob-

* Read before the Physical Society, March 21, 1874.

jection may be taken. This will be best accomplished by collecting the statements of its principal supporters, and arranging together their explanations of the phenomena which arise—(1) when dissimilar metals at the same temperature are placed in contact; (2) when one insulated metal is placed in a liquid capable of acting chemically upon it; (3) when two different metals are placed insulated and unconnected in one such liquid; (4) when the two metals are joined across by a metallic arc, or when two or more cells are joined up in series.

1. That the contact of metals is always attended with the production of a difference of potential between them was for a long time denied by ardent supporters of the chemical theory. De la Rive endeavoured to show that the effects observed might be attributed to oxidation; but his experiments are not conclusive, and to Sir W. Thomson belongs the credit of having established the fact by experiment, irrespective of his theoretical deductions from the facts of thermo-electricity. He thus describes his decisive experiment:—"A metal bar insulated so as to be movable about an axis perpendicular to the plane of a metal ring, made up half of copper and half of zinc, the two halves being soldered together, turns *from* the zinc *towards* the copper when positively electrified, and *from* the copper *towards* the zinc when negatively electrified." The difference of potential he finds to be about 0·6 or 0·7 of that of a Daniell's cell when the metals are perfectly clean; but by oxidation of the copper it may be made equal to or even greater than that of a Daniell's cell. He has also shown that if zinc and copper cylinders be connected by a wire, the electrometer detects a difference between the potentials of the air in the interior; and, lastly, that if copper filings be allowed to fall from a copper funnel in contact with a vertical zinc cylinder, they convey a negative charge to a receiver placed below. Sir W. Thomson concludes that there is sufficient evidence to show that zinc and copper attract one another chemically at any distance if connected by a fine wire, and that, as Prof. Tait remarks, "when *any* two bodies of different kinds are brought into contact, there is a certain amount of exhaustion of the potential energy of chemical affinity between them, and that the equivalent of this is—partly, at least—developed in the new potential form of a separation of the so-called electric fluids, one of the bodies receiving a positive, the other a negative charge, the quantity depending on the nature and form of the bodies." This is equivalent to saying that at the surface of contact there is perpetually a force tending to separate the two electricities in a direction perpendicular to that surface, while at all points ever so little within it there is no such force.

Prof. Maxwell reiterates essentially the same facts. He remarks that the electromotive force, as determined by this method experimentally, does not account for the whole electromotive force of a simple couple. This latter is in general far greater than that given by the Peltier effect for the same pair of metals. "Hence the greater part of Volta's force must be sought for, not at the junctions of the two metals, but at one or both of the surfaces which separate the metals from the air or other medium which forms the third element of the circuit. Prof. Jenkin, referring to these experiments of Thomson, adds that "In cases where no known chemical action occurs, as where zinc and copper touch each other, and yet difference of potential is produced, since this involves a re-distribution of electricity, a small but definite consumption of energy must then occur: the source of this power cannot yet be said to be known."

2. It seems to be universally admitted that when an insulated metal is placed in a liquid capable of acting chemically upon it, a difference of potential is produced between the metal and the liquid, a sudden rise in potential taking place in passing from the metal surface to the liquid in contact with it, or that the metal becomes negatively and the liquid positively electrified, metals differing in the degree of electrification they can produce with any one electrolyte.

3. But if we ask what are the conditions when two different metals are so immersed, we find the most contradictory statements given. Sir W. Thomson expressed his opinion thus in 1862:—"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's. I now think it quite certain that two metals dipped in one electrolytic liquid will (when polarisation is done away with) reduce *two* dry pieces of the same metals, when connected each to each by metallic arcs, to the same potential,"—which seems equivalent to saying that there is no difference of potential produced other than that due to dissimilar contact. Thus also Prof. Tait:—"By interposing between two metals which have been electrified by contact a compound liquid or electrolyte, these metals are at once reduced to the same potential—a result which could not have been obtained by connecting them by any metallic conductor. By the passage of the electricity a portion of the electrolyte is decomposed, and the potential energy thus developed is equal to that possessed by the electricity while separated in the metals." Prof. Jenkin advocates essentially the same views:—"When two dissimilar metals are plunged side by side into a liquid, such as water or dilute sulphuric acid, they do not exhibit *any* sign of electrification; the three materials remain at one potential, or nearly so. If, while the two dissimilar metals are in the liquid, they are joined by metallic contact to terminal pieces of one and the same metal, these terminal pieces will be brought to the same difference of potentials as that which would be produced by direct contact between the dissimilar metals." This amounts simply to saying that, as long as no wires are attached to the plates of a single cell, there is no difference of potential; but that when wires are joined on, the observed difference of potential is due to the contact of the *wire* with that metal plate to which it is dissimilar. Again:—"When a single metal is placed in contact with an electrolyte, a definite difference of potentials is produced between them; zinc in water becomes negative, copper in water becomes negative, but less so than zinc. If however, the two metals are plunged *together* into water, the copper, zinc, and water forming a galvanic cell, all remain at one potential, and no charge of electricity is observed on any part of the system." "If a piece of copper be now joined to the zinc, it (the copper) will become negative, and the other copper plate positive, the difference of potentials being that due to the direct contact between the zinc and piece of copper *only*, the water having the effect of

simply conducting the charge from the zinc to the copper plate, and maintaining them at one potential."

The foundation for these statements is found apparently in an experiment due to Sir W. Thomson. He finds that if half-discs of zinc and copper be arranged under a movable metallic needle maintained at a high positive potential, if they are connected by a wire or by contact, the needle moves in such a way as to show that the copper is negative and the zinc positive; while if they are separated by a slight interval, and connected by a drop of water, *no* difference of potential is observed. Prof. Jenkin also lays great stress on the fact that, whereas copper in contact with zinc becomes negative, in a single cell with wires attached it is the wire attached to the zinc that shows a negative potential. This he holds to be conclusive that the junction of the wire with the zinc plate is the real seat of the electrical separation; although he admits that there may be a slight difference due to the liquid, and that different liquids may augment or decrease this difference.

In another place he says:—"If the voltaic theory of the cell were absolutely correct, the electromotive force of the cell would depend wholly on the plates in the electrolyte, and not at all on the solution employed to connect them." But it has been found that the potential series of the metals is slightly changed by the solution employed to join the plates: in order to account for this fact it is necessary to treat the voltaic theory as incomplete. He adds, however, that the potential series of the metals for water, dilute acids, and ammonic chloride, do not differ so much as to invalidate the theory, although the series for alkaline sulphides is quite different and anomalous.

4. When the two plates in one electrolyte are joined by a wire, or when simple cells are joined up in series and the circuit closed by a wire, we find it stated that there is a constant separation of the electricities at the point of contact of different metals, and a constant re-combination, attended with decomposition, through the electrolyte. "Perhaps it is strictly accurate to say that the difference of potential is produced by the contact, and that the current which is maintained by it is produced by chemical action." And, lastly, that in a series of cells the electromotive force is due to the sum of the differences of potential produced by all the contacts.

(To be continued.)

MATHEMATICS
FOR
NATURAL SCIENCE STUDENTS.

By T. C. SIMMONS, B.A.

(Continued from p. 253.)

Logarithms.

The student by this time understands, I hope, that the logarithm of a number between 10 and 100 lies between the integers 1 and 2, that the logarithm of a number between 100 and 1000 lies between 2 and 3, and so on. The integral part of a logarithm is called its *characteristic*, and the fractional part its *mantissa*. It will be seen that the characteristic of the logarithm of a number between 10 and 100 is 1, while the number of digits in the number is of course 2. Thus the characteristic is one less than the number of digits in the number. This holds always. For instance, the number 3542 contains 4 digits, so that we at once conclude that its logarithm has 3 for its characteristic.

But how about the logarithms of numbers less than 1? To explain this it is necessary to show what is meant by *negative* indices. If we divide 10^2, say, by 10^3, we get 10^1, the 2 being obtained by subtracting 3 from 5. But if, instead of this, we were to divide 10^2 by 10^5, we are then said to get 10^{-3}, the -2 being obtained by subtracting 5 from 3. Thus we denote by the symbol 10^{-2} that number which is equivalent to the quotient $\frac{10^2}{10^5}$. This number is of course $\frac{1}{10\,10\,10}$, or $\frac{1}{10^3}$. Thus 10^{-3} denotes $\frac{1}{10^3}$. So 10^{-1} denotes $\frac{1}{10}$, 10^{-4} denotes $\frac{1}{10^{-4}}$, and so on. The same will apply to fractional indices. By $10^{-\frac{1}{2}}$, for example, we mean $\frac{1}{10^{\frac{1}{2}}}$, or $\frac{1}{\sqrt{10^3}}$.

Now if a number lie between $\frac{1}{10}$ and 1, its logarithm will of course lie between -1 and 0. (I ought perhaps to explain that 10^0 is taken to denote 1, for this reason:—

$$10^3 \div 10^3 = 10^{3-3} = 10^0.$$

And $10^3 \div 10^3 = 1$. Thus $10^0 = 1$.

It might have been shown by employing any other number than 3). Thus the logarithm will be $-1 +$ some fraction. The characteristic then is -1. The mantissa of a logarithm is *in all cases* considered to be positive. The negative sign is not then generally placed *before* the characteristic, but *above* it, to show that it does not apply to the mantissa. Thus the logarithm of ·3 is expressed thus:—

$\bar{1}$·3010300. If a number lie between $\frac{1}{100}$ and $\frac{1}{10}$ its characteristic will be -2, for the same reason. Such a number will commence with a single 0 directly after the decimal point,—as, for instance, the number ·05164. The rule for obtaining at once the characteristic of a fractional number is this:—Express it as a decimal, then add 1 to the number of 0's which follow immediately the decimal point, prefix the negative sign, and the characteristic is obtained.

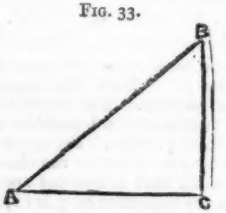
Now the sine and the cosine of an angle are always less than 1. If therefore we took strictly the logarithms of sines or cosines, we should always have negative characteristics. This would be a little inconvenient, and it is usual to always add 10 to the logarithms of these ratios. Thus the sine of 30°, for instance, is $\frac{1}{2}$, or ·5, and its logarithm is $\bar{1}$·6989700. This is negative; by adding 10, however, it becomes positive, and is 9·6989700. This is called the *tabular logarithm* of sin 30°, and is denoted by L sin 30°. Thus L sin 30° = 9·6989700, log sin 30° being, of course, $\bar{1}$·6989700. The same method is used in the case of the cosine. Thus we avoid the negative characteristic.

I will here show how, when a certain number of logarithms have been calculated, the logarithms of all other numbers may be deduced. We know that

when a number is multiplied by 10 its logarithm is increased by 1, when multiplied by 100 the increase is 2, and so on. Similarly when a number is divided by 10, 100, 1000, ... its logarithm is diminished by 1, 2, 3, ... respectively. Now let us suppose we have a table of logarithms calculated for all numbers from 1 to 100,000, and let us require the logarithm of 12·324, say. It will not be found in the tables, but if we look for the log. of 12324 we shall find it to be 4·0907517. Now $12{\cdot}324 = 12324 \div 1000$, and thus log. $12{\cdot}324 =$ log. $12324 - 3$. Thus the required logarithm is 1·0907517. I need not give any more examples: they will be found in abundance in any collection of mathematical tables. If there be more than five digits in a number the logarithm is obtained by interpolating between two logarithms given in the tables; but this I need not explain here.

The reader must by this time have become weary of so much discussion on the theory of logarithms. I shall now show some of their practical applications. I shall return to a problem which I stated could be done by trigonometry in my first paper on the subject. Suppose that we wish to find the height of a church tower, or any other vertical (or, as it is commonly called, *perpendicular*) object, and that we cannot ascend it. If we measure a length A C along the ground from C, the foot of the object B C, we can, by the aid of a sectant, or by

Fig. 33.



other means,, measure the angle C A B. Suppose, for example, A C to be 73 feet, and C A B to be 49°.

$$\text{Then } \frac{B\,C}{A\,C} = \tan B\,A\,C,$$

$$\text{or } B\,C = A\,C \tan B\,A\,C,$$

$$= 73 \times \tan 49°.$$

$$\text{Hence log. } B\,C = \text{log. } 73 + \text{log. } \tan 49°,$$

$$= \text{log. } 73 + \text{L } \tan 49° - 10,$$

the tabular logarithm being, as I have explained, the ordinary logarithm + 10.

Now from the tables we find—

Log. 73 = 1·8633229
L tan 49° = 10·0608369

Add, then subtract 10, and we get 1·9241598. Thus the length of B C given in feet is expressed by the number whose logarithm is the above result. This number, which we look for in the tables, is 83·977. Thus the height of B C is nearly 84 feet.

I must caution the reader always to remember that the logarithms of the trigonometrical ratios given in tables are always the tabular logarithms, and that to obtain the real logarithm of any ratio we must subtract 10.

We might, instead of looking in the table of Logarithmic Tangents, have looked in that of Natural Tangents. We should have found—

Tan 49° = 1·15 nearly.

This, if multiplied by 73, produces 83·9. Thus the height is nearly 84 feet, the same as we found before. If we require only an approximate result it will be better to obtain it by direct multiplication or division, as in the above example; but if we require it to be correct to 5 or 6 places of decimals it will save a vast amount of trouble if we use logarithms. Again, suppose we know the height of B C, and wish to find A C. It is possible that a marsh or a river may lie between A and C, and we have no means of measuring A C directly. We proceed thus:—

$$A\,C = B\,C \cot B\,A\,C,$$

$$= 84 \times \cot 49°,$$

$$= 84 \times 0{\cdot}87,$$

as found from the tables,—

$$= 73 \text{ nearly.}$$

Or thus:—

$$\text{Log. } A\,C = \text{log. } 84 + \text{L cot } 49° - 10.$$

Log. 84 = 1·9242793
L cot 49° = 9·9391631

Sum after subtracting 10 = 1·8634424

And this is found to be the logarithm of 73 nearly.

I might add other examples, but I think that those I have given will be found sufficient to enable the reader to understand the application of logarithms to trigonometrical problems. He has now reached a point where he finds his knowledge useful. I would advise him to work out problems in logarithms by himself, testing their correctness by direct methods. For instance, let him multiply sin 65° by sec 32°. He may do this by adding the tabular logarithms of the two ratios, and subtracting 20. The result will be the logarithm of the required product. He may then look out from his tables sin 65° and sec 32°, and multiply them, say to two places of decimals. The two results ought, of course, to coincide. I would advise him always to remember that if he wants his result correct to a certain number of places he must work to a greater number, in order to secure correctness. For instance, if he requires his result correct to two places, he should work with four places of decimals, and then at the end cut off the last two digits. I noticed some time ago a striking instance of how one place of decimals affects another. I was calculating, by the aid of the series, log. 2. I found it to be 0·3010299. In all the tables, though, it is given as 0·3010300. If I had carried on my calculations to 8 places instead of 7, I should no doubt have found that something had to be carried from the 8th to the 7th place. This would have made the result, of course, 0·3010300. Thus in this instance, in order to get the 5th decimal place correct, it is necessary to calculate for 8 places. This is, I admit, an exceptional instance; still it is always necessary to be careful. It will generally be sufficient if we calculate for two places more than we require for our result.

I shall in my next article discuss several properties of triangles, and relations subsisting between the sides and angles, and shall show how to find the area of a triangle, having given some of its sides and angles.

The number of messages (of twenty words) passing over the Barcelona-Marseilles cable, for the month ending July 31, 1874, were 6382, against 5461 for June.

ON THE

CHANGE OF RESISTANCE IN HIGH TENSION FUZES AT THE MOMENT OF FIRING.*

By Major MALCOLM, R.E.

Occasionally on having a fuze in circuit with a delicate reflecting galvanometer of 1360 ohms resistance in bridge, the author noticed a regular vibration of the light, continuing for a considerable time and as often as the current was turned on, so as to lead to the belief that the motion was due to no vibration of the room or other external cause, but solely to the varying passage of the current through the substance of the fuze, which seemed to discharge itself as if by the motion of some of its particles.

In the dynamo-electric machine of Messrs. Siemens is found, in a not very weighty form, a machine capable of firing 120 high tension fuzes in continuous or plain circuit and 110 in two parallel circuits. Another much lighter has fired 36 such fuzes in six parallel circuits, showing that the spark was capable of a subdivision which the Austrian frictional machine of Von Ebner is quite unable to approach. This machine also fired 49 fuzes in continuous circuit.

100,000 ohms may be considered a fair average resistance of the ordinary high tension Abel Land Service fuze made without any special precautions.

Some fuzes greatly exceeding this limit, while others fall much below it, it became a matter of interest to discover in what lay the tremendous power which enabled these dynamo-electric machines to overcome such enormous resistances with heat enough to explode the fuzes. For this purpose a hundred Leclanché cells, size No. 3, were prepared, and a circuit arranged commencing from one pole of the battery, through a fuze whose resistance had been previously taken as before indicated, passing on through a Gaugain tangent galvanometer to the battery by a fork-tailed lead, so that the number of cells could be increased conveniently without breaking the circuit or originating disturbances of the needle. The needle had been made very short and light with a glass pointer fixed on to it.

The author began always with one or two cells, and then by the forked-tailed lead worked slowly up the battery till the fuze exploded, noting carefully the deflection of the needle of the galvanometer until explosion. He then substituted such metallic resistance in the circuit as with the number of cells which had produced explosion reproduced the noted deflection.

The conduct of some of the fuzes was strangely different from that of others. For instance, one which gave no indication of a current from two cells, the resistance suddenly broke down before the current from twenty-one cells, giving a violent deflection at moment of firing only.

The author proceeds:—But generally the deflection increased very fairly with the increased number of cells applied, as I tried to keep the influence of each degree of current power as nearly equal as possible; occasionally, however, the amount of deflection *fell*. One case noted resistance 450; number of cells to fire fuze, 9; deflection of galvanometer 5°, after giving a permanent deflection of 80°; the metallic resistance required to reproduce the deflection noted here is 1320 ohms.

In another case—Resistance 320; cells 8; deflection 5°, after deflection of 45°; metallic resistance 1440.

A satisfactory explanation of this variation of conduct would be exceedingly interesting.

I may quote a few instances of the general law. (See Table.)

What follows, besides explosion, after the resistance of a fuze has by the energy of about 43 cells been reduced from 37,000 ohms to 4950 the author does not pretend to say; but having watched the resistance fall down so low, he thinks it fair to suppose that in some cases with inconceivable rapidity the resistance is still further broken down until the current can easily be conceived to be sufficiently great to heat the inflammable substance of which the fuze is composed to the exploding point.

Resistance.	Cells.	Deflection.	Metallic Resistance sufficient to reproduce Deflection.
490,000	18	1°	17,000
15,000	10	11	750
499,000	16	16	640
425	10	6	1350
1550	5	15	265
9550	35	10	3750
10,500	39	8	5500
10,500	38	10	3990
37,000	43	10	4950

Experiments with High Tension Fuzes, March 5, 1874.

Resistance of Fuzes.	No. of Cells Fuze Fired with.	Gaugain Tangent Galvanometer. Resist. 0·75 Ohm.	Reproduced Resistance.
490,000	18	1	18,000
15,000	10	11	780
420,200	20	*Nil.*	—
499,000	16	16	680
425	10	6	1380
1550	5	15	270
550	7	12	440
490,000	21	3½	5250
450	9	5	1350
680	10	10	840
4400	15	1½	4700
320	8	5	1450
Infinite	21	*Nil.*	—
650,000	22	8	2300
19,000	13	*Nil.*	12,000

High Tension Fuzes (Newest Pattern).

Resistance of Fuzes.	No. of Cells Fuze Fired with.	Deflection on Gaugain Tangent Galvanometer.	Resistance at moment of Firing.
95,000 B.A.U.	35	10	3750 B.A.U.
10,500 ,,	39	8	5500 ,,
10,500 ,,	38	10	3990 ,,
37,000 ,,	43	10	4950 ,,
13,000 ,,	21	12	1980 ,,
30,000 ,,	26	6	4400 ,,
17,000 ,,	30	8	4600 ,,
7,000 ,,	36	11	3800 ,,

The Chairman (Mr. Latimer Clark) asked whether the compound in the fuse was in the form of a powder or of a solid.

Prof. Abel said that it consisted of a mixture of phosphide of copper and subsulphide of copper, very specially prepared with a view to permanence, together with chlorate of potash as the oxidising agent. These formed an explosive mixture, which served also for conducting the current, and offered a very high resistance. The mixture was compressed with great pressure. It was found that no great care was required in the manufacture of fuses sufficiently sensitive to be fired by means of high tension machines. The high tension fuses which had been more recently made for submarine purposes were made somewhat more carefully. They were carefully compressed until some clear resistance was obtained on the galvanometer, but the same care was not necessary with ordinary high tension fuses. The apparently anomalous results furnished upon the examination of the fuses with a battery such as that of Leclanché, might be ascribed to the fact that the mixture was a mechanical one, and varied somewhat in different fuses. The measure of resistance of the fuse was by no means an indication

* Abstract of a paper read before the Society of Telegraph Engineers.

of its sensitiveness to ignition. If there existed a very minute quantity more of subsulphide of copper in one fuse than in another, one fuse would be much more conducting than the other, and consequently the galvanometer indications in the two fuses would be very different. On the other hand, the fuses would vary in sensitiveness, because one would contain more of the comparatively very sensitive phosphide of copper than the other, and would be more readily lighted by heat. Hence the galvanometer indications would not correspond with the amount of battery power required by the two fuses. He could not now explain why there should be a drop in the galvanometer at the moment of firing, but he believed that it would be explicable upon further investigation. The variations as regarded sensitiveness and conductivity were due not to one cause only, but to two causes, which operated in opposite directions.

Mr. W. H. PREECE asked what power was used, and how the resistance of the fuse was taken; and also whether the sudden failure of the galvanometer was noticed at the moment of explosion, and was continuous after the explosion.

The CHAIRMAN said that Mr. Preece had asked almost the same question which he wished to ask, and that was, whether they could reduce the fuse in its deflection to a higher conductivity and a lower resistance, and maintain it there.

Major MALCOLM said that he did not think that the fuses were practically irregular. The reason he had brought the subject forward was that some time ago there was a talk in that room about atmospheric electricity and the sudden break down of resistance in the face of great electrical power. He had found that in this mixture—no matter what differences of resistance there might be—there was the same sort of quelling of resistance in the face of the power. That was the point which he had ventured to bring before the Society.

Mr. W. H. PREECE enquired whether, when the needle fell, it fell permanently.

Major MALCOLM said that the needle did not fall. The action was too quick for the galvanometer to show it. He did not know how to trace it, and should be glad if some one would show him. If the resistance instantly fell to zero, it must do so in such an infinitesimal fraction of time that he had no means to enable him to follow it.

Prof. ABEL said that the action of the passing current in the fuse was to develop heat, and as that heat was raised the conductivity of the material was increased, and the deflection fell. While the needle was falling the heat reached a certain point, and then fell. The resistance might be very much less in one fuse than in another, because the amount of heat required to explode the composition in one fuse would be different from that in another fuse. For that reason there would be a very different fall of the galvanometer in one case from that in another. Major Malcolm had endeavoured to measure, as rapidly as possible, the deflection at the instant before explosion, and that varied, as he (Prof. Abel) had said, with the amount of heat developed, and with the sensitiveness of the material to explosion by heat.

Mr. W. H. PREECE remarked that he thought that Major Malcolm had started a very interesting branch of enquiry. He had alluded to some observations that were made when a paper was read on atmospheric electricity. That paper was written and read by himself (Mr. Preece), and he therein endeavoured to point out that one of the causes which induced lightning protectors to act in the way they did was that at the moment a flash of lightning struck across a layer of air it converted that layer of air, for the time being, into a circuit of no resistance, and that hence the whole of the current due to atmospheric electricity passed across this path of no resistance in preference to going through the coil of the instrument, which coil possessed some resistance. Since then he had endeavoured to pursue this enquiry, and to trace it out, and nothing but want of time had prevented him from combining into a paper the great mass of experiments that had been made on that point. The experiments consisted of watching the effect of increased electromotive force upon the resistance of a medium. That was precisely the same point as that which Major Malcolm had endeavoured to illustrate. The experiments had been made in this way:—Layers of air, both *in vacuo* and otherwise, had been arranged by means of micrometer screws so that the distance could be varied at pleasure. The electromotive force had been varied at pleasure from one cell to six thousand cells. By these means there had been made a series of experiments which showed that even in the case of air, and in the case of any ordinary insulating medium, as the electromotive force was increased the resistance of the body was reduced. In the case of air the variation followed a definite law, which he hoped one day to put in a form suitable for communication to the Society. No doubt the same thing occurred in the case of fuses. It was seen in the case of the dielectric insulator of submarine cables, where variable results were obtained according to the power that was used to test. If Major Malcolm would pursue his enquiries among fuses, and if those who had opportunities of testing submarine cables to a large extent, and of testing vacuum tubes and layers of air, would also pursue investigations on the subject, he was sure that they would find a definite law which determined the resistance of bodies which opposed currents, just as they now were acquainted with the law which applied to bodies that favoured the passage of currents.

In reply to a question as to whether the explosion of the fuses was due to electrolytic action, and the chemical effect of one substance on another, independently of mere heat,—

Prof. ABEL said that he had no doubt that such an action took place, but of course the explosion must be referred partly to heat. Probably one assisted the other. No doubt when heat had been developed to a certain extent, chemical action began, and that probably assisted the reduction of the resistance in the fuse. And there was no doubt that the development of heat was a consequence of the development of chemical action. This would explain the fact that when fuses were subjected to this severe ordeal they seldom returned to their original resistance.

Electrical Science in English and Foreign Journals.

(In addition to our usual abstracts, there will be found under this heading the titles of papers on electrical sciences which have appeared in English and foreign journals from the commencement of the present century up to the date of the establishment of this journal).

Poggendorff's Annalen der Physik und Chemie.

VOL. XLI. (1837).

Harris's Electrical Experiments in Rarefied Air. P. 99.
Researches on the Properties of Magneto-Electric Currents. M. de la Rive. P. 152.
On the Thermo-Electric Spark. Mr. Wheatstone. P. 160.
On the Production of Thermo-Electric Currents between Metals and Melted Salts. Mr. Andrews. P. 164.
Clarke's Magneto-Electric Machine. P. 223.
On an Apparatus for Showing Volta's Fundamental Experiment. M. Fechner. P. 225.

Observations on the Magnetic Declination, Inclination, and Horizontal Intensity at Mailand in 1836-7; from the Data of a New Inclinatorium. M. v. Humboldt. Pp. 521, 528.

VOL. XLII. (1837).

On Production of Electricity from Chemical Combinations. M. Dulke. P. 91.
On the Influence of Heat on Facility of Passage of Electric Currents through Liquids and Metals. M. de la Rive. P. 99.
Voltaic Combinations. M. Daniell. Pp. 263, 267.
On the Voltaic Pile, and the General Law for the Intensity of Currents of a Simple Circuit, and a Pile of greater or less Tension. M. Pouillet. P. 281.
On the Relative Measurement of Thermo-Electric and Hydro-Electric Sources, and on the Quantities of Electricity Required for Decomposition of 1 grm. of Water, and for Producing greater or less Commotions in Fixed Circumstances. M. Pouillet. P. 297.
Description and Use of the Electro-Magnetic Balance, and the Constant Current Battery. M. Becquerel. P. 307.
Justification of the Contact Theory of Galvanism. M. Fechner. P. 481.
On Thermo-Electricity Generally; on the Thermo-Electric Spark and a Thermo-Electro Magnet. Mr. Alexander. P. 624.

VOL. XLIII. (1838).

On the Heating in Closed Circuits of the Electric Battery. M. Riess. P. 47.
Observations on the Electromotive Behaviour of some Hyperoxides, Platinum, and Iron. M. Schönbein. P. 89.
Notice on Various Electric Phenomena. M. Sellier. P. 187.
Researches on Electricity, with special reference to the Theory of the Galvanic Circuit. M. Munck af Rosenschold. Pp. 193, 440.
Experiments on the Use of Blue Vitriol Solution, and Iron Plates for Voltaic Batteries. Mr. Fyfe. P. 228.
New Observations on Voltaic Currents Excited by Chemical Affinities. M. Schönbein. P. 229.
Magnetic Observations at Mailand in the latter half of 1837. M. v. Humboldt. P. 292.
Notice on a New Electro-Dynamic Apparatus wrought by Terrestrial Magnetism. M. v. Kramer. P. 304.
On the Power of Certain Flames and Hot Air to Conduct Electricity. Mr. Andrews. P. 310.
Some Experiments on the Theory of Galvanism. M. Fechner. P. 433.
The Induction-Inclinatorium. M. Weber. P. 493.
Magneto-Electric Apparatus for Production of Induced Currents of Equal Intensity in Wires Completely Separated from each other. M. Dove. P. 511.

VOL. XLIV. (1838).

Attempt of an Electro-Chemical Theory. M. Gmelin. P. 1.
Attempt of a Theory of Galvanism. M. Fechner. P. 37.
On the Electric Intensity of the Insulated Pile. M. Fechner. P. 44.
Some Remarks on Fechner's "Justification of the Contact Theory of Galvanism." M. Schönbein. P. 59.
On Galvanic Combinations. M. Delffs. P. 78.
One Word More on Electric Distribution, and the very Remarkable Phenomenon on an Electricity Become Free through Repulsive Force without Power of Propagation. M. Pfaff. P. 332.
Some Experiments in the Region of Galvanism. M. Lenz. P. 342.
On the Behaviour of Blue Vitriol Solution in the Galvanic Circuit. M. Lenz. P. 349.
On Chemical Decompositions through Simple Hydro-Electric Apparatus. M. Becquerel. P. 537.
On the Becquerel Circuit. M. Pfaff. P. 542.
Description of a Thermo Pile for Constant Currents. M. Dove. P. 592.
Remark on the Power of Propagation of Bound Electricity. M. Riess. P. 624.
Experiments on Thermo-Electric Currents. M. Matteucci. P. 629.
On the Galvanic Spark. M. Jacobi. P. 633.
On the Significance of the Law of Fixed Electrolytic Action for the Theories of Voltaism. P. 642.

(To be continued).

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences, Vol. lxxviii., No. 26. June 29, 1874.

On Electrostatic Phenomena in Batteries.—By M. Alfred Angot.—The author states that since between each battery element there exists a *constant* difference of potential, varying only with the nature of the substances in contact and their temperature, we have a fundamental principle sufficient to establish in a simple manner the theory of electrostatic phenomena in the battery. Place a conducting body of electric capacity, C, in remote communication with one of the poles of a battery, at the potential V, which we will suppose insulated. The potential diminishes to v at that pole; but it falls at the same time to the same quantity in all the battery, for the difference of potential from one couple to the following should be constant and independent of the absolute value of this potential. So, though the potential at any point of the battery varies from the couple following, the contact of a conducting body produces in every point of the battery an even potential fall, precisely like in a conducting body. We may then speak of the electric capacity of a battery as similar to that of a conducting body, which is the quantity of electricity necessary to lower or elevate, from the unit, every point of the battery. Hence the same equations which solve the problem of the division of electricity between two conductors are applicable to the division of electricity between a battery and a conductor. Regarding the capacity of the battery, at the moment when in the light of the electric division it conducts itself absolutely like a conducting body, no reason can be seen, *a priori*, why it *should* be different from a conductor of the same dimensions: this may be mathematically proved thus:—Let P be the capacity of the battery defined as explained above, C that of the conducting cylinder of the same dimensions, and E that of the body in communication. The potential of the pole being V, and falling to v by reason of contact with E, E take a charge M, such that $M=(V-v)E=Pv$, whence we deduce—

$$v=V\frac{E}{P+E}, \text{ and } M=V\frac{PE}{P+E}.$$

If, on the other hand, the cylinder C at the same potential is placed in communication with the same body E, the potential falls to v', and the cylinder C takes a charge M', when the equations for M' and v' are the same if we substitute C for P. If by the conditions of any experiment $M=M'$, then $C=P$; that is to say, the capacity of a battery, such as has been defined, is expressed by the same number as that of the conducting cylinder of similar dimensions. The experiment to test this was as follows:—A Volta column on insulating supports was used and the negative pole put to earth for a moment. The potential of the positive pole was then represented by n elements of the battery, which was placed in a distant communication with a very sensitive, Branly's, electrometer.

The deflection δ_1 of the electrometer was proportional to its charge M, so that the quotient $\frac{\delta_1}{\pi}$ represented the value—

$$\frac{M}{V}=\frac{PE}{P+E},$$

E being the electrometer's capacity. Further, a conducting cylinder was placed in communication with a Volta battery of 100 elements, which gave it a potential equal to 100; afterwards it was placed in communication with an electrometer; the deflection of this latter was proportional to the charge M', which it then took and gave the value—

$$\frac{M'}{V}=\frac{CE}{P+E}$$

Vol. lxxix. No. 1. July 6, 1874.

First Note on Electric Transmissions Across Ligneous Bodies.—By M. Th. Du Moncel.—See article on "*Electric Conductibility of Ligneous Bodies*" in the TELEGRAPHIC JOURNAL for August 1, 1874.

No. 2. July 13, 1874.

Second Note on Electric Transmissions Across Ligneous Bodies, included in the article "*Electric Conductibility of Ligneous Bodies*" referred to above.

Journal of the Telegraph. Vol. vii., No. 14. July 15, 1874.

Curious Electrical Phenomenon.—During the past three nights the engineers and machinists at the round house of the Virginia and Truckee Railroad Company, in this city, have been in a state of lively excitement about some strange electrical disturbances which have occurred there regularly every night. For three nights all hands have been engaged in trying to solve the mystery, but have only partially succeeded. The company erected a large smoke-stack, which rises to a considerable height above the roof of the round house. At the base this smoke-stack is spread out in the shape of a funnel. It was built in order that the locomotives may stand under it to cool off when they come in off the road, and also while being fired up in the morning. Into the side of this smoke-stack, three or four days ago, was run a stove-pipe from a stove standing in a room about 40 feet distant. It is about this stove that the electrical disturbances take place. The first that was observed of these was about 11 o'clock, four nights since, when a machinist approached the stove for the purpose of putting a stick of wood in it. As the stick neared the stove he received such a shock that it fell from his hand, and his arm was benumbed. He at first thought he had taken a sudden cramp in his arm. In trying again to put the wood into the stove he received a second shock. This time he observed a flash and heard a sharp snap, which he at once recognised as a discharge of electricity. They tried all manner of experiments, and found that the stove was fully and heavily charged with electricity, it giving out sparks and flashes when a piece of iron, steel, wood, or the naked hand approached it. After an hour or two the phenomenon ceased. The next day nothing was seen of it; but the next night, about 11 o'clock, shortly after a locomotive came in and stopped under the funnel of the smoke-stack, the stove was again charged, and began snapping and cracking, sending out sparks and flashes on all sides. So it has been every night since. They now know that the heated locomotive causes this electrical display, but in just what way is that which is still puzzling them.

Les Mondes. Vol. xxxiv., No. 13. July 30, 1874.

Electric Conductibility of Ligneous Bodies.—M. du Moncel.—See TELEGRAPHIC JOURNAL, vol. ii., p. 261.

Philosophical Magazine. Vol. xlviii., No. 316. August, 1874.

General Theory of Duplex Telegraphy.—Louis Schwendler.—This system, which was known in 1854, was allowed to lie idle for about eighteen years, for the following reasons:—(1). The invention was in advance of the age. (2). The telegraph profession, young as it is, is far more conservative than is good for the advance of telegraphy, and, on the whole, telegraph administrations and staffs have by no means that professional education which is required to conduct practical experiments with a clear understanding, and thence deduce rational conclusions. Thus prejudice was created, which was increased from year to year by authors of school literature writing most discouragingly on the subject. (3). Unfortunately during all that time no physicist found it worth his while to investigate the duplex methods with a view to ascertain quantitatively what can be expected of them, and how they actually compare, with respect to safety, with single working. (4). Duplex working itself could not progress, because it was neither tried nor investigated, and hence no suggestions for overcoming the difficulty of charge and discharge were called for. The remainder of the paper is a mathematical solution of one of the methods of "duplex" working based upon the following two conditions: if these conditions could always be fulfilled the system would be entirely on a par with single telegraphy. The conditions are—1. *The receiving instrument of each station should not be affected by its own sending.* 2. *The duplex signals and single must be of equal strength.* "Duplex" signals occur when the times of sending them from the two stations fall together. Single signals, however, are when the moments of sending from the two stations do not fall together, and the signals are produced as in ordinary telegraphy. It will be clear, therefore, that when the two stations are at work at the same time, "duplex" and "single" signals must necessarily follow each other in accidental succession. Nay, one and the same signal produced may be partly a duplex and partly a single signal. Theoretically, the two conditions named are always fulfilled by every duplex method, otherwise it would have to be rejected. Practically, however, the different methods may behave very differently with respect to their fulfilment; for, in practice, variations—especially in virtue of the line having by no means a constant electrical condition—are constantly going on. These unavoidable variations, it is clear, may cause very different quantitative disturbances of the two conditions, either if we compare different methods or the same method under different resistance arrangements. Two of the problems investigated are—1. What is the best arrangement of any given duplex method when the line is regarded as a variable conductor, but not as acting perceptibly as a Leyden jar? 2. What must be the distribution of condensers along a given resistance in order that the two essential conditions may be least disturbed for a speed of signalling variable between two fixed limits? The enquiry in the present number relates to the solution of the first problem with respect to the "bridge method," and the general results are that the branches of the bridge, with the exception of the one lying opposite the line, must be equal to each other, and severally equal to half the measured conductor resistance of the line. Secondly, the branch lying opposite the line should be equal to the sixth part of the measured conductor resistance of the line, and only in this—the smallest of all the branches—should re-adjustment of balance be made. From the development of these general results it will be evident that they fulfil the following conditions:—

I. The irregularity of signals in the one station is entirely independent of the irregularity of signals in the other station.

II. The irregularity of signals in each station is due only to balance not having been rigidly established.
III. If balance in either station is disturbed, a single adjustment in the branch will re-establish that balance.
IV. Any disturbance of balance will have the least possible effect on the received signals.
V. Maximum current at balance.
VI. Maximum magnetic effect of the maximum current on the receiving instrument.

The author promises a continuation of this paper.

City and Commercial Notes.

In his circular, dated August 6th, Mr. W. Abbott says:—Although the extent of business in telegraph securities has been limited, prices have exhibited a stronger tendency, and in most instances show an advance, after deducting the quarterly dividends recently distributed. The two settlements of the month have revealed a preponderance of purchases on behalf of the investing public, and thus the amount of floating stock has been considerably reduced. A noticeable feature this month has been the further advance in value of the Globe shares, both preference and ordinary, which again demonstrates with what favour this amalgamation of telegraph property is regarded by investors. It is anticipated that by the first week of next month the Great Eastern will have completed the new line for the Anglo-American Company, which will then possess four cables in perfect order. So much has been said of the watering of the Anglo stock at the time of the amalgamation with the French Atlantic Company, that it may be as well to repeat that this last addition to the property of the Company will not involve any increase in the capital, and that on the repair of the 1865 cable, next year, the Company will have five cables at work, with established connections in all parts of the world. With such prospects as these the depreciation of £2,100,000 on the par value of Anglo-American stock is certainly not justified.

The decline in the traffic for July over the Eastern system is to be accounted for by the interruption of the two lines to Lisbon for a period of ten days; but for this exceptional occurrence a large increase would, no doubt, have been shown.

From recent advices there is evidently no lack of telegraphic development going on in all parts of the world. From Japan we learn that heavy cables of English manufacture are being submerged by the Japanese Government, and from the Argentine Confederation the Minister of the Interior reports that the Republic has now 4146 miles of telegraph open for traffic. The advantages of an extended system of internal telegraphs have long been admitted by the authorities of the Argentine Republic, and this official statement of the progress made will be especially welcome and encouraging at the present time to the holders of Brazilian Submarine and other cable property connected with South America, as internal telegraphs are feeders to, and provide business for, the submarine lines. Besides this, what has been done in the River Plate will no doubt also be accomplished by Brazil.

The proprietors of submarine telegraph property will, no doubt, have noticed with considerable interest that the question of the Government purchase of the British Ocean telegraphs is likely to come into prominence under auspices of some influence. Mr. Reed, M.P., late Constructor of the Navy, has placed upon the notice paper of the House of Commons a motion which he will submit next session, "to call attention to the desirability of bringing British Ocean telegraph lines under united and responsible public management, and in closer co-operation with the telegraphic systems of the Continent, and that a Select Committee be appointed to inquire and report upon the subject." There may be many and various opinions as to the policy of the purchase of these properties by the Government. International as well as Imperial interests are involved in it, but there is no doubt it is high time that the question should be impartially and thoroughly investigated thus early, as delay only adds still more to the difficulties surrounding it.

The Western and Brazilian Telegraph Company, Limited, have received intimation that the land lines connecting Rio Grande do Sul with Monte Video are completed and open for traffic. Communication by telegraph is thus established between Europe, the River Plate, and the West Coast of South America.

The number of messages passing over the Cuba Submarine Telegraph Company's line during the month of July amounted to 1758, estimated to produce £2000, against 1078 messages producing £1065 in July last year.

The Eastern Telegraph Company's traffic receipts for the month of July, 1874, amounted to £27,247, and to £28,975 in the corresponding period of 1873.

The traffic receipts of the Eastern Extension, Australasia, and China Telegraph Company, Limited, for the month of July, 1874, amounted to £19,641, and to £18,190 for the corresponding period of 1873.

The traffic returns of the Great Northern Telegraph Company for the month of July were—this year, 410,501 francs; last year, 271,873. Total traffic receipts, 1st January to 31st July—this year, 2,499,909 francs; last year, 1,722,112 francs.

TELEGRAPH SHARE LIST.

Amount per Share.	Name of Company.	Amount paid up.	Closing Quotations.
£		£	Aug. 14.
Stock	Anglo-American (Limited)	100	71½—72
10	Brazilian Submarine..	All	7½—7⅝
10	Cuba	All	5⅝—6⅛
10	Direct Spanish	9	..—..
20	Direct United States Cable	All	13—14
10	Eastern (Limited)	All	7⅞—8⅛
..	Do., New	..	104—106
10	Eastern Extn. Australia and China	All	7⅞—8
10	Globe Telegraph and Trust	All	6—6½d.
10	Do., 6 per cent Pref...	All	9—10xd.
10	Great Northern	All	10½—11⅛
25	Indo-European	All	16—16½
10	Mediterranean Extension (Limited)	All	4—4⅛
10	Do., 8 per cent Pref...	All	10½—11⅛
10	Panama and South Pacific	2½	..—.. dis.
8	Reuter's..	All	10½—10⅝
Stock	Submarine	100	210—220
1	Do., Scrip	All	1⅞—2⅛
10	West India and Panama	All	3⅞—4⅛
10	Do., 10 per cent Pref.	All	10½—10⅝
20	Western and Brazilian (Limited) ..	All	11—11⅛
1000 dls.	West Un. U.S. 7 per cent 1st M.B.	All	102—104
10	Hooper's Telegraph Works	All	12½—12⅝
50	India-Rubber and Gutta Percha ..	All	19—22
Cert.	Submarine Cables Trust	100	104—107
12	Telegraph Construction	All	27½—28
100	Ditto Ditto 7 per cent Bonds	All	103—105

Patents.

Applications for Letters Patent.

2462. A. V. Newton, of Middlesex, Mechanical Draughtsman, for an invention of "Improvements in the manufacture of electric batteries."—A communication to him from abroad by G. L. Leclanché, of Paris. *Dated July 14, 1874.*

2478. W. H. Davies and F. H. W. Higgins, both of London, for an invention of "Improvements in telegraphic signalling instruments for transmitting a number of fire and other alarms or calls upon a single wire." *Dated July 15, 1874.*

2539. W. E. Newton, of Middlesex, Civil Engineer, for an invention of "Improvements in chemical telegraphs and in apparatus connected therewith."—A communication to him from abroad by T. M. Foote and C. A. Randall, of New York, U.S.A. *Dated July* 20, 1874.

2625. T. Slater, of London, Electrician, for an invention of "Improvements in electro-magnets, and in batteries or apparatus to be employed therewith." *Dated July* 28, 1874.

2637. G. K. Winter, F.R.A.S., Telegraph Engineer of the Madras Railway, of Arconum, in the Presidency of Madras, British India, for an invention of "Improvements in electrical inter-communication in trains." *Dated July* 28, 1874.

2646. J. H. Johnson, of Middlesex, for an invention of "Improvements in the method of and apparatus for transmitting musical tones by electricity."—A communication to him from abroad by E. Gray, of Chicago, U.S.A. *Dated July* 29, 1874.

2648. A. M. Clark, of Middlesex, Patent Agent, for an invention of "Improvements in electro-magnetic governors for steam heating and drying apparatus."—A communication to him from abroad by J. M. Bradford, of Portland, Maine, U.S.A. *Dated July* 29, 1874.

2659. R. K. Boyle, of New York, U.S.A., now of Southampton Buildings, London, for an invention of "Improvements in electric telegraph apparatus." *Dated July* 30, 1874.

NOTICES TO PROCEED.

822. J. Imray, of Middlesex, has given notice in respect of the invention of "Improvements in electric telegraph sending apparatus."—A communication to him from abroad by C. Lemon, of Wellington, New Zealand. *Dated March* 6, 1874.

862. J. Imray, of Middlesex, has given the like notice in respect of the invention of "Improvements in laying telegraph cables or other insulated electric conductors, and apparatus therefor."—A communication to him from abroad by T. J. Waters, Japan. *Dated March* 10, 1874.

954. A. M. Clark, of Middlesex, Patent Agent, has given the like notice in respect of the invention of "Improvements in electric telegraphy, and in apparatus for the same."—A communication to him from abroad by G. d'Infreville and W. N. Armstrong, both of New York. *Dated March* 17. 1874.

964. E. F. Kingsley, of Middlesex, has given the like notice in respect of the invention of "An improved telegraph."—A communication to him from abroad by M. Gally, of Rochester, New York. *Dated March* 19, 1874.

2235. E. Oesterley, of London, has given the like notice in respect of the invention of "Improvements in duplex telegraphy, and in apparatus to be used for this purpose."—A communication to him from abroad by L. Schwendler, of Calcutta, India, electrician. *Dated June* 27, 1874.

UNITED STATES.

149,763. C. L. Le Barron, Pensacola, Fla. *Telegraph insulator.* Case A. Dated December 31, 1873.—The insulator provided with a slot for the line wire, formed at a right or nearly right angle to a slot for a spike, and extending below the latter, as and for the purpose specified.

150,090. G. S. Shute, Boston, Mass. *Electric and thermostatic fire alarm.* Dated January 3, 1874.—Two series of thermostats, each acting, through rack and pinion, on wheels having projections which, coming in contact, close circuit. Thermostats act differentially, causing this contact. Two thermostats in each series act, when quiescent, as springs against each other, steadying the apparatus.

150,150. J. M. Goodwin, Cleveland, Ohio. *Electric railway signal.* Dated July 14, 1873.—The line wire of a railroad telegraph is carried along a bridge or beam supported above the track, and upon this beam is arranged a key for breaking the circuit. Upon the top of a car is a rock shaft arranged lengthwise the car, and having attached to it a series of fingers, which, when projecting upward, strike the key as the car passes under the beam. The fingers may be adjusted to break the circuit as desired for signalling, or, by means of the rock shaft, may be turned so as not to strike the key.

151,102. V. H. Forville, St. Petersburg, Russia. *Portable telegraph apparatus.* Dated April 9, 1873.—Intended to be carried on railroad trains for use by those unskilled in telegraphy, being placed on brackets attached at intervals to posts.

150,953. G. W. Howe, Stevenson, Ala. *Printing telegraph.* Dated June 20, 1873.—Type in a reciprocating rack. Proper type, when in position, thrown up against paper. Six keys used, the combinations of which control all the types through one, two, or three pulsations acting in different magnets.

151,157. T. L. Reed, Providence, R.I., assignor of one half his right to E. F. Phillips, Providence, R.I. *Electric conducting cordage.* Dated December 15, 1873.—1. Electric cordage for switch boards, and electrical therapeutic apparatus composed of a metallic conductor, closely coiled with a tubular interior, and embraced or enclosed in and by an insulating jacket of braided or woven fibrous material. 2. The combination, with a spirally coiled metallic conductor, of a terminal tip, which is provided with a neck for entering the tubular interior of the coil and a shoulder for connection therewith.

151,209. T. A. Edison, Newark, N.J. *Automatic telegraphy and perforators therefor.* Dated September 2, 1873.—Roman letters formed by perforations. Depression of one key selects and actuates the required punches, from a number thereof, to form the letter corresponding to such key. Message is chemically printed at receiving end in Roman letters.

151,335. F. F. A. Achard, Paris, France. *Electro-magnetic car brake.* Dated January 15, 1874.—Circular magnets on an axle suspended by pivoted frame; circuit closed thereto; they are attracted against axle of wheel, which communicates rotary motion to the magnets and drums attached thereto, break chains being wound up on latter.

151,750. J. Buchtel, Portland, Oregon. *Electric signal apparatus for fire hose.* Dated April 1, 1874.—Sections of both wires always connected, and both helices at each coupling protected by a single sheath. In a fire telegraph the insulated wires embedded in the hose, and having their sections connected by the double coil enclosed in the case.

To Correspondents.

*** Duly authenticated contributions, theoretical and practical, on every subject identified with the interests of which "THE TELEGRAPHIC JOURNAL" is the organ, will always command attention. Anonymous correspondence will be wholly disregarded. Literary communications and books for review should be addressed to the EDITOR; business communications to the PUBLISHER.

SUBSCRIPTIONS.

Copies of the journal may be obtained through newspaper agents in every part of the world. Should any difficulty arise in procuring copies, direct communication with the PUBLISHER is requested.

Gentlemen desiring to subscribe and receive this paper regularly, are requested to send a remittance to the Office at Boy Court, Ludgate Hill, London, E.C., for 9s., if residing in the United Kingdom; if in Africa, Australia, Belgium, Brazil, Canada, Egypt, France, Gibraltar, Jamaica, Malta, Mexico, Monte Video, Natal, New Zealand, Sweden, United States, West Indies—10s.; if in Austria, Ceylon, China, Holland, India, Italy, Japan, Portugal, Germany, South America (West Coast), Spain, Switzerland, Turkey, Valparaiso—12s.; if in Russia, 14s.

Cheques and Post Office Orders to be made payable to HENRY GILLMAN, and to be crossed "London and County Bank."

ALL SUBSCRIPTIONS PAYABLE IN ADVANCE